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JUVENILE KELPS AND THE RECAPITULATION THEORY. II

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II. THE RECAPITULATION THEORY IN RELATION TO THE KELPS

ANY observations on juvenile kelps must call to mind the recapitulation theory. This theory, though applied both to animals and to plants, was built up exclusively on zoological evidence and has been amplified and discussed chiefly by zoologists. The reason is evident because of the definite proportions and structure of the animal body, the development of which must of necessity follow a very definite course, while among plants the body is of such loose and indefinite proportions that its development can seldom be rigidly described. But while the botanists have had very little to say about the recapitulation theory, they have always approved it and considered that it applied to plants just as truly, though not as conspicuously, as to animals.

It is somewhat surprising then to a botanist to find that this theory is being very vigorously attacked by some of the zoologists. One of the more recent papers is by Montgomery, who gives a review of the literature with a general discussion of the theory in his "Analysis of Racial Descent," 1906. In summing up he says (p. 193):

> Therefore we can only conclude that the embryogeny does not furnish any recapitulation of the phylogeny, not even a recapitulation marred at occasional points by secondary change. . . . An analysis of the stages during the life of one individual can in no way present a knowledge of its ancestry; and the method of comparing non-correspondent stages of two species is entirely wrong in principle.

And again at the close of the chapter, p. 203:

> The recapitulation hypothesis is scientifically untenable and where there has been transmutation of species, the embryogeny neither in

> whole nor in part exactly parallels the racial history. The relation between them is always that of an inexact parallelism. Considerations based on any such idea of recapitulation are erroneous, and therefore of no help in determining racial descent.

In these sentences Montgomery is voicing not alone his individual opinion, but that of a very considerable school of embryologists.

The general tenor of these statements is scarcely open to question nor is the author's conclusion as to the worthlessness of the recapitulation theory. However, there is one word used in both the paragraphs quoted, though not in the portion of the first cited, that is unfortunate in that it is open to misunderstanding. It is the word exact. Exact has a certain mathematical flavor, which makes its application to living organisms difficult. Neither Montgomery nor any one else believes that there are anywhere two individuals, who are exactly alike in any respect whatever. We may fairly assume, that Montgomery means to say that there is no recapitulation of the racial history of the embryo sufficiently exact to aid in determining racial descent; and we shall so interpret his statements in the remainder of this paper.

A few years ago when the recapitulation theory was almost universally accepted one might have assumed that the noteworthy features of the development of the kelps were to be explained on that basis. But now in the face of such attacks on the theory no such assumption may be made. We shall therefore consider the development of the kelps in relation to the theory and to the criticism upon it in an effort to ascertain the real bearing of the foregoing observations.

It must be admitted that the juvenile forms of all the kelps are closely similar in a general way; but it does not necessarily follow that they are so because of any recapitulation of phylogeny. Such parallelism might be brought about by entirely different causes. This possibility has been perhaps most strongly urged by His, the eminent embryologist, who in a different way makes quite as strong an attack on the theory as does Montgomery. In his "Unsere Körperform" as translated and quoted by Morgan ('03, p. 71) who does not, however, assent, His says:

> In the entire series of forms which a developing organism runs through, each form is the necessary antecedent step of the following. If the embryo is to reach the complicated end forms, it must pass, step by step, through the simpler ones. Each step of the series is the physiological consequence of the preceding stage and the necessary antecedent for the following. Jumps, or short cuts, of the developmental process, are unknown in the physiological process of development. If embryonic forms are the inevitable precedents of the mature forms, because the more complicated forms must pass through the simpler ones, we can understand the fact that paleontological forms are embryonal, because they have remained at the lower stage of development, and the present embryos must pass also through lower stages in order to reach the higher. But it is by no means necessary for the later, higher forms to pass through embryonal forms because their ancestors have once existed in this condition. To take a special case, suppose in the course of generations a species has increased its length of life gradually from one, two, three years to eighty years. The last animal would have had ancestors that lived for one year, two years, three years, etc., up to eighty years. But who would claim that because the final eighty years species must pass necessarily through one, two three years, etc., that it does so because its ancestors lived one year, two years, three years, etc.? The descent theory is correct in so far as it maintains that older, simpler forms have been the forefathers of later, complicated forms. In this case the resemblance of the older, simpler forms to the embryos of later forms is explained without assuming any law of inheritance whatever. The same resemblance between the older and simpler adult forms would remain intelligible were there no relation at all between them.

There are two ways of looking at this view of His's that every form is the necessary antecedent of the succeeding. These depend upon the length of stages considered. If we take stages separated by very small intervals of growth, His's contention must be true else there would be no continuity of development. But this is nothing more than a statement of the fact that all growth must be gradual and is no law of development. If instead of small intervals we take the whole development, the statement would become: "The developmental stages of an organism are only the physiologically necessary steps for the formation of its adult body from its earliest stage, which is in most cases the egg." This is definite and it can be readily tested by the facts, while the other is so vague as to be scarcely susceptible of any such test. There is no middle ground between these two alternative interpretations of the statement. For if an organism is found to which it will not apply if somewhat but not greatly separated stages be considered, all that is necessary is to take shorter and shorter stages until finally any ontogeny must conform to it.

Let us apply then, His's view, thus interpreted, to the kelps. We have so far confined the account to the external morphology and have said little about their histology. This will be of interest here. The general plan of structure is the same in both stipe and lamina and similar in all kelps. Within the epidermis is the cortex composed of polygonal or rounded cells which may be thickened and hardened to form strengthening tissue. Within this is a pithweb made up of irregularly interlacing filaments which sometimes show very remarkable differentiation. Oliver ('87) first worked out in detail, showing that in Macrocystis and Nereocystis, especially, sieve tubes are developed which form a regular zone of vertical vessels around the less differentiated center of the pith. The sieve plates of these become obliterated by the formation of callus as in the spermatophytes. There is good reason to believe that they are efficient in the transfer of materials from one part of the plant to another and their possession may have made it possible for these plants to attain the great lengths they sometimes reach. The simpler internal pith consists of interlacing branching hyphae which run in all directions. Many of these meet and at their junctions develop sieve plates connecting them with one another, at the same time becoming swollen at the ends like the flare of a trumpet. Such trumpet hyphæ are common in most members of the Laminariaceae. In Renfrewia, however, the pith consists of only moderately elongated cells which interlace somewhat as in other kelps, but very much less conspiciously. The majority of them are not longitudinal, but transverse in their general course, so that a cross section shows more of them cut lengthwise than a longitudinal (see figures, Griggs, '06). Scarcely any of them are sufficiently elongated to merit the name of hyphæ. Very few give indications of developing into trumpet hyphæ. It is evident that Renfrewia presents a transition from a pithweb of simple polygonal cells to the complex differentiation of the higher kelps such as Nereocystis. Such plants must of necessity pass through the condition of Renfrewia in order to attain mature structure. We have here then a perfect illustration of the truth of His's idea—save in one respect. His contends that the developmental stages are *only* the necessary morphological precursers of the adult. But in this case they may be phylogenetic recapitulations also. There is nothing in the evidence so far to prevent a decision either way.

Let us consider some other features of the development. All of the young forms pass through a period when the stipe is short as compared with the lamina. In all which have been described above except Hedophyllum, this condition persists until a certain very definite period, after which the stipe elongates rapidly (see figures of Egregia). This condition is so similar to the adult stage of Renfrewia that one is tempted to consider it a recapitulation of such a stage. But instead it may be only a necessary physiological adaptation which the young plant undergoes early in its development in order to provide a large photosynthetic area to furnish the food necessary for rapid growth. *A priori* this would seem a reasonable interpretation of the facts and it may be that we should consider them without other significance. It is, however, difficult to believe that the simple Renfrewioid form is the necessary precurser of adult forms so diverse as Postelsia and Egregia, Eisenia and

Nereocystis, Thallasiophyllum and Macrocystis. One might imagine other forms upon which each of these might have been built up more directly than on this one. This is particularly true in the case of Egregia and Hedophyllum, where, while the young are indistinguishable, the course of development is diametrically opposed. Egregia dwarfs the lamina and becomes nearly all stipe; Hedophyllum obliterates the stipe and becomes a sessile lamina. If ontogeny represents merely stages physiologically necessary to the attainment of the adult form, why should Hedophyllum produce a stipe at all?

Similar conditions are presented by very many other cases, especially among animals where some organ is developed in the embryo which later disappears without being of service either to the embryo or to the adult. Such cases have in the past been the main evidence brought forward for the recapitulation theory, as it has been supposed they were explicable only on the basis of a recapitulation of the phylogeny. Familiar examples are cited by Morgan (see below), and many more might be added.

Not all who attack the recapitulation theory go so far as to discard it altogether. Many recognize in it a truth and seek to modify it to fit certain facts. The form which has the largest number of adherents is perhaps that proposed by Morgan ('03), who believes that animals in their ontogeny repeat not the adult, but the embryonic stages of their ancestors; that the presence of a certain structure in the embryo means that the ancestors of the species to which the organism belongs had similar embryonic stages. This he calls the "Repetition Theory." Much of the evidence which the zoologists bring forward in favor of such a modification as against any broader application is so conclusive, one must acknowledge that such is a correct statement of the facts in the particular cases cited, whatever the general law of development may be. Morgan calls attention to the fact that the gill-clefts and the notochord, structures on the recurrence of which the recapitulation theory was largely built, appear just as early in the embryo of the fish and of Amphioxus, respectively, as in that of a mammal. He cites the case of the baleen whale which forms teeth in the embryo like any other mammal, but these beginnings, instead of continuing their development, are absorbed and do not even pierce the gums. The same is true of the dental ridges of birds, where teeth begin to form but soon disappear.

The evidence presented by the kelps clearly tends to establish this repetition theory of Morgan. The juvenile forms of the plants have so many points in common that there can be scant doubt but that their ancestors had similar juvenile forms. It must be added here also that those plants whose development we have traced above are not special cases, but are only illustrations of the facts common to all kelps. The writer has in his possession full series of several genera which have never been described at length. These and all others which have been worked out follow the same course of development. Among those upon which fairly complete published data are available, may be mentioned: Agarum, Barber, '89; Alaria, Schrader, '03, and others; Cymathere, Griggs, '07; Eisenia, Setchell, '96b, '05a; Lessonia, Reinke, '03; Nereocystis, MacMillan, '99; Pterygophora, MacMillan, '02; Saccorhiza, Barber, '89; Thallasiophyllum, Setchell, '05a.

If we may consider the repetition theory established how much will it help us with our phylogenetic problem? Why should widely diverse forms have ancestors with similar embryos? How were these similar stages acquired and why do they persist? They must be meaningless so far as phylogeny is concerned, except as they are considered as stages which once led to the development in the adult of the structures which they represent. But why should embryonic characters persist and not adult ones? Is there any line of demarkation between embryo and adult beyond which the action of heredity changes?

Leaving these questions for the present, we may examine the facts in the development of our kelps, to ascertain whether these juvenile forms repeat only other juvenile forms or whether they go farther and approximate the adults of their ancestors. Nothing could be more instructive on this point than the figure of the young plant of Lessoniopsis printed beside the adults of Renfrewia (Figs. 15–17). In all external characters save the characteristic spots of Lessoniopsis and the reproductive maturity of Renfrewia they are in essentials identical. The structure of the holdfast is particularly interesting. Both are simple discs strengthened by primary hapteres originating through the uneven growth of the disc itself. The young of other kelps might have been used for this comparison, *e. g.*, Hedophyllum (cf. Fig. 6), but Lessoniopsis retains these primitive characters at a larger size than the others and therefore lends itself more easily to photography while its determination is at the same time certain because of the spotted lamina. In Pterygophora the correspondence is in all respects just as complete, see MacMillan's figures ('02). There persists for a considerable period the simple lamina with the short stipe on the primitive disc and its primary hapteres for holdfast. After the secondary hapteres have appeared and until the midrib has been formed the young plants are very difficult to distinguish from those of *Laminaria saccharina* which grows in the same locality. These again are in all respects, except size and reproductive maturity, like the adult plants of their species.

It seems obvious that we can not well consider these facts without comparing these non-correspondent stages of Lessoniopsis and Renfrewia, and of Pterygophora and Renfrewia and Laminaria. The simple facts of the case are that Lessoniopsis and the others when still very small pass through a condition which must be considered within the generic limits of Renfrewia. Conversely, the adults of Renfrewia do not differ in any important characters save size and reductive maturity from the young of the other kelps which have been studied. But Renfrewia, juvenile or adult, is not one of the ancestors of these higher kelps. It is only a simpler form which we take to have been left behind in the evolution of the kelps. Our actual knowledge of their ancestors is almost nothing. But if we were to reconstruct a generalized common ancestor for the kelps, by projecting backward, from the different tribes, lines indicating their apparent course of evolution, until they converged and met, we should have to conceive a plant very similar in all respects to Renfrewia.

What then is to be said concerning structures which do not recapitulate adult but only embryonic conditions? In the toothless animals, the whale and the bird, the development of teeth in the jaw is entirely unnecessary, as has been pointed out in considering His's idea. It may even be said to hinder the attainment of the adult condition. The same is true of the mammalian gill-slits and of most of the structures which have in the past attracted attention in connection with the recapitulation theory. As the ancestral period, when such structures were fully developed in the adult, becomes more and more remote, the tendency to inherit them becomes less and less, because of the cumulative impulses given to the heritage by the nearer ancestors. Consequently, they are successively less and less developed. Any gradual loss of inherited structures can, in the nature of the case, take place only from the mature condition backward towards the beginning of the life cycle; otherwise we should have adult structures with no ontogenetic history. Therefore we can understand why it is that in many cases only the embryonic stages of ancestral organs persist in the ontogeny.[3]

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[3] The cutting off of end stages in the development of organs has given rise to the idea that the adult stages are "pushed back into the embryo." Such a misconception easily arose from the loose language in which the facts have often been expressed. Conklin ('05) has rightly pointed out its incorrectness.

Thus the embryogeny will be gradually shortened by the omission of more and more of the superfluous ancestral stages; and it will tend finally to retain only such stages as are necessary to the attainment of the adult form. It will be noted that this is the view of His, which thus becomes a statement of an inevitable tendency in development, which is very different from a complete law of embryogeny. Though life cycles may approach very closely such a limiting condition, it is doubtful if they would ever completely realize it.

Besides changes in ontogenies brought about by the cutting off of end stages no longer used there is another source of change. This is secondary adaptation. It is on this point that Montgomery largely makes his case, insisting that organisms are as subject to change in one period of their life cycles as another. In this matter also we must agree that secondary changes are sometimes very evident and conspicuous—probably more so among animals than plants. The fetal membranes are very familiar examples of such secondary adaptations. But though they are much modified the fact must not be lost sight of that they are in part at least adaptations of previously existing organs with different functions and not new structures. Not only may an embryo adapt itself to its conditions; it may simulate other forms; or interpolate stages; or become otherwise modified as the species undergoes transmutation. Yet the important point to consider is not that a few have done this, but that the great majority have not falsified their heritage beyond all recognition, that they still persist in spite of changed conditions and secondary adaptation in preserving so many indications of their ancestry.

Montgomery considers this matter of secondary change so weighty, not because of a great amount of observation brought forth, but for logical reasons. He holds that:

> The egg of a mammal is as dissimilar from that of a fish as their adult stages, no matter whether their differences are perceptible or not. This was the idea of the great old master Von Baer: The egg is as much a bird as is the hen.

Although perfectly true in a physiological sense, this is incorrect in this connection. Potentially the egg of one animal is as different from that of another as their adult forms, but morphologically they correspond. Morphology is not concerned with the "growth energies" of organisms, but only with their form and structure. A similar mistake was made by His in the quotation cited above, where he takes for an illustration of his views an animal which had lengthened its life over that of its ancestors. The logical deduction from such an example under the recapitulation theory would be that the last form should *die* at the end of each period, one, two, three years, etc., in order to recapitulate its ancestry, rather than that it *lives* one, two, three years to do so. The absurdity of this lies in the fact that length of days is not a morphological character. The recapitulation theory has nothing to do with physiology; it is purely a matter of morphology.

The degree of approximation between the young of a higher form and the adult of a present-day lower form of the same line depends upon the degree of specialization and divergence of the lower species from the main path of descent. It is usually recognized that most of the lowest and morphologically simplest organisms are highly specialized for some particular mode of life more or less different from the ancestral. This specialization nearly always carries with it some structural adaptations, but these may not obscure the ancestral characters. Thus Marchantia has evolved a chambered thallus highly differentiated, to adapt it at once to an aquatic substratum and aerial life, but it still retains a sporophyte perhaps very similar in some features to that of the ancestors of the higher plants at the liverwort stage. On the other hand, organisms are occasionally found which give every indication of being primitive. These are truly forms with arrested evolution. Renfrewia is an example; Anthoceros is another, less free from specialization but contrasting strongly with Marchantia. Such primitive types are few and far between for obvious reasons: if an entire group advances rapidly it moves up bodily into a higher plane and leaves behind only such forms as stray into some byway of specialization, which specialization would be a bar to future progress except in the line upon which the form had entered. All unspecialized forms left behind in the advance of the race are likely to be displaced early in the struggle for existence because of their lack of particular adaptations. It is accordingly only in such environments as present no specialized demands upon their inhabitants that we may expect to find these primitive forms and it will be observed that to a large extent such is the case.

Wherever a form is found with simple unspecialized structure it becomes at once a problem to decide whether it is in reality primitive or a degenerate type. If there is no paleontological history to aid in the solution a conclusive answer to this question is often impossible. However, unless there is definite evidence of degeneration in vestigial structures or the like, as there is in many cases, for example the mistletoes; it is generally safe to assume that the present condition of the organism represents its highest attainment in the process of evolution. Degenerate forms usually manifest a high degree of fixity in their organizations and great variability is seldom found in such forms. It might be suggested that the apparently primitive structure of Renfrewia may be due to degeneration from a condition more highly differentiated. It possesses, however, no vestigial or unused organs, with the exception of the basal cone of the stipe. Every portion of the plant is functional. There are no peculiarities about its structure which mark it as different from the other kelps. On the contrary, its reproduction and its histology are similar to them. Its habitat, quiet water just below the tide mark, is exactly that which would be expected of the ancestors of the kelps before they acquired adaptations enabling them to endure the heavy surf and the drying incident to living above the tide mark. At the same time it has such a high degree of variability in its whole structure that it is difficult to pick out characters sufficiently fixed to be of use in describing it. There seems to be no good reason to doubt its primitive position.

Taking all the evidence into consideration, it seems to the writer that we are bound to conclude that though organisms are subject to adaptation at any stage of their life cycles and may gradually cut out superfluous stages, yet, except as some such tendency has operated to change the heritage, the development of the individual does recapitulate the history of the race. The degree of correspondence of any individual cycle with its ancestral history is various in different cases but may be very close. Recapitulation must take place if there is any force which tends to make offspring like parent, if heredity is of any importance in moulding the forms of organisms. On the other hand, if there be any variability or transmutation of individuals in stages other than the adult end stages of their life cycles, the recapitulation can not be perfect, but must be marred at every stage where secondary change has taken place. The extent to which any individual will recapitulate its phylogeny must therefore depend on the balance maintained between these two forces in the given case. The value of a study of ontogeny for the taxonomist or phylogenist will depend altogether on the facts of the special case. In each case the evidence must be weighed before a conclusion can be reached. Ontogeny may be of greater or less worth in the attempt to build a rational system of nature. But variable as its utility may be in different cases, the recapitulation theory states a fundamental law of a tendency of the embryogeny and must be considered as one of the several interacting tendencies which together control the development of animals and plants.

COLUMBUS, O., August, 1908.

LITERATURE

No attempt is here made to list the voluminous literature devoted to discussions of the recapitulation theory. The more important papers have been thoroughly listed and summarized in many of the standard general works, *e. g.*, Montgomery.

Barber, C. A. On the Structure and Development of the Bulb in *Laminaria bulbosa*. *Ann. Bot.*, 3, 41. 1899.

Conklin, E. G. The Organization and Cell Lineage of the Ascidian Egg. *Jour. Acad. Nat. Sci. Philadelphia*, vol. 13. 1905.

Frye, T. C. *Nereocystis lutkeneana*. *Bot. Gaz.*, 42, 143–146, fig. 1. 1906.

Gepp, A. and E. S. *Lessonia grandifolia*. *Jour. Bot.*, 43, 2. 1905.

Grabendörfer. Zur Kenntnis der Tange. *Bot. Zeit.*, vol. 43. 1885.

Griggs, R. F. *Renfrewia parvula*, New Kelp from Vancouver Island. *Postelsia*, 1906, 247–274, pls. 16–19. 1906.

————. Cymathere, a Kelp from the Western Coast. *O. Nat.*, 7, 89–96, pl. 7, fig. 1. 1907.

Hooker, J. D. Flora Antarctica, 2, t. 171, 167. 1844.

Humphrey, J. E. On the Anatomy and Devolpment of *Agarum turneri*. *Proc. Am. Acad.*, 23, 201. 1886.

Kjellman, F. R. Laminariaceæ in Pflanzenfamilien, 1^2, 242–260. 1893.

MacMillan, C. Observations on Nereocystis. *Bull. Torr. Club*, 26, 273–296, pls. 361–362. 1899.

————. Observations on Lessonia. *Bot. Gaz.*, 30, 318–334, pls. 19–20. 1900.

————. The Kelps of Juan de Fuca. *Postelsia*, 1901, 193–220, pls. 22–26. 1901.

————. Observations on Pterygophora. *Minn. Bot. Stud.*, 2, 723–741, pls. 57–62. 1902.

Montgomery, T. H. The Analysis of Racial Descent in Animals. New York. 1906.

Morgan, T. H. Evolution and Adaptation. New York. 1903.

Oliver, F. W. On the Obliteration of the sieve tubes in the *Laminariæ*. *Ann. Bot.*, 1, 95. 1887.

Postels and Ruprecht. Illustrationes algarum. 1840.

Ramaley, Francis. Observations on *Egregia Menziezii*. *Minn. Bot. Stud.*, 3, 1–9, pl. 1–4. 1903.

Reinke, J. Studien zur vergleichende Entwickelungsgeschichte der Laminariaceen. Kiel. 1903.

Saunders, D. A. Algæ of the Harriman Alaska Expedition. *Proc. Wash. Acad. Sci.*, 3, 391–486, pls. 18–62. 1901.

Schrader, Herman F. Observations on *Alaria nana*. *Minn. Bot. Stud.*, 3, 157–166, pl. 23–26. 1903.

Setchell, W. A. Concerning the Life History of *Saccorhiza dermatodea*. *Proc. Am. Acad.*, 26, 177–217, pls. 1 and 2. 1891.

————. Classification and Geographical Distribution of the Laminariaceæ. *Trans. Conn. Acad.*, 9, 333–375. 1893.

————. Notes on Kelps. *Erythrea*, 4, 41–48, pl. 1. 1896a.

————. *Eisenia arborea*. *Erythrea*, 4, 129–133, pl. 4; 155–162, pl. 5. 1896b.

———. The Elk Kelp. *Erythrea*, 4, 179–184, pl. 7. 1896c.

———. *Laminaria sessilis* in California. *Erythrea*, 5, 98. 1897.

———. Notes on Algæ, I. *Zoe*, 5, 121–129. 1901.

———. Post Embryonal Stages of Laminariaceæ. *Univ. Cal. Pub. Bot.*, 2, 115–138, pls. 13–14. 1905a.

———. Regeneration among Kelps. *Ibid.*, pp. 139–168, pls. 15–17. 1905b.

———. Nereocystis and Pelagophycus. *Bot. Gaz.*, 45, 125. 1908a.

———. Critical Notes on the Laminariaceæ Nuov Notarisia 19: 90–101. *Rev. Jour. Roy. Mic. Soc.*, 1908, 474. 1908b.

Setchell, W. A., and Gardiner, N. L. Algæ of Northwestern North America. *Univ. Cal. Pub. Bot.*, 1, 165–418, pls. 17–27. 1903.

Sykes, Miss M. G. Anatomy and Histology of Macrocystis and *Laminaria saccharina*. *Ann. Bot.*, 22, 291–325, pls. 19–21. 1908.

Thuret. Recherches sur les Zoospores des Algues et les Antheridies des Cryptogames. *Ann. Sci. Nat.*, ser. 3, 14, 240, t. 30. 1850.

DeToni, J. B. Sylloge Algarum, 3, 316–374. 1895.

Wille, N. Beitr. Physiolog. Anatomie Laminariaceanum. *Univ. Fests. til. K. M. K. Oskar*, II, Anleidung Regjieringsjubilaeet. 1897.

Williams, J. L. Germination of the Zoospore in Laminariaceæ. *Nature*, 62, 613. 1900.

Yendo, K. On Eisenia and Ecklonia. *Bot. Mag. Tokyo*, 16, 203. 1902.

———. Two new Marine Algæ from Japan. *Ibid.*, 17, 99–104, pls. 2 and 3. 1903a.

———. Hedophyllum spirale sp. nov. *Ibid.*, 17, 165–173, pl. 6. 1903b.